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OF

ELECTRICAL ENGINEERS.

SPECIAL MEETING, JANUARY 8TH, 1889.

SUBJECT:

Lightning Arresters and the Photographic Study of Self-Induction.

BY E. G. ACHESON.

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AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS.

VOL. VI. New York, March, 1889. No. 3.

The thirty-first meeting of the Institute was held at the College of the City of New York, 17 Lexington Avenue, on Tuesday, January 8th, 1889. The meeting was called to order by the Secretary at 8 P.M.

On motion of Mr. T. C. Martin, Mr. Francis W. Jones was elected Chairman of the meeting.

THE CHAIRMAN:—Gentlemen, as there is no routine business to be transacted, I will not keep you waiting, but will avail myself of the pleasure of introducing Mr. E. G. Acheson, Member of the Institute, who has kindly volunteered to give us a paper upon Lightning Arresters and the Photographic Study of Self-Induction.

Mr. Acheson then read the following paper:

LIGHTNING ARRESTERS AND THE PHOTOGRAPHIC STUDY OF SELF-INDUCTION.

BY E. G. ACHESON.

If in the opinion of any an apology is required—and it is thought that such will be the case—for thus presenting a subject that is in any of its parts a reproduction of the experimental work of such authorities as Professor Hughes, Professor Lodge, Dr. Hertz and others, the excuse is offered that the original motive for entering into the series of experiments referred to in this paper was a legitimate one, namely—the clearing away of a little fog surrounding some annoying phenomena attending the working of cables of electric conductors. The investigations became so interesting, so striking and suggestive, and some of the results appeared to have such a value, that it was believed they would afford sufficient excuse for this presentation.

While the measurements made during these investigations were not so accurate as it would be possible to obtain with more refined instruments, the probable errors were not so great as to make them wholly valueless, care in the readings and many determinations checking to a great extent the errors due to the crudeness of the apparatus. Appreciating the fact that untruthful results are less desirable than none, owing to their misleading effects, it is intended to impress the fact that the determinations contained in this paper can at best only serve as a possible guide for future work of a more complete and accurate character. It is also to be hoped that they may promote the already lively interest displayed by the scientific world in discharges, and the sometimes oscillatory character of electric currents.

The mass of material and composite character of the work to be presented will necessitate considerable care in selecting the order of presentation, and it is believed that the subject can be

more easily followed if the thoughts and experiments succeed each other in the order in which they occurred to the experimenter, and this plan will be followed even when the after thought is of the greater importance.

As previously stated, the primary motive from which followed this series of experiments was the clearing up of some uncertainty as to the cause of a few annoying troubles in electric cables. But, to be more precise, it was desired to determine the cause of the occasional "grounding" and failure of cables used in telegraphic and telephonic service, when to all outward appearances they were perfect in construction, protected with lightning arresters, and immediately before the moment of grounding indicated high insulation resistances. A case of this kind presented itself for solution, in which there were some features of peculiar attraction to an investigator. It was a cable containing five wires about one mile in length, and of that design known as anti-induction, the cross-section being star-shaped, each conductor having a surrounding wall of lead. It was used for telegraph work, and the conductors were connected at the ends to overhead lines, lightning arresters being provided for the protection of the cable; the terminals of the cable were on poles some twenty feet in height. The ground plates of the arresters were connected to the earth in the usual manner; that is, by wires running down the poles to plates in the ground.

This cable was continually getting into trouble, the conductors becoming solidly "grounded," and upon examination it was found that the "grounds" were caused by little kernels or spots of carbonized insulation. It was at the first moment thought that these had been introduced in the manufacture of the cable, but their repeated formation emphatically declared against this theory and made it imperative to learn the true cause. It was not probable that they were produced by the working currents, for they were of comparatively low electromotive forces, while the presence of the lightning arresters would, in accordance with the prescribed rules of their action, render it wholly unlikely that they could have been produced by lightning.

A close study of the experiments of Professor Hughes of self-induction, and of Professor Lodge on the "alternative path" had produced an unsettled condition of my ideas as to the real value of a lightning arrester. The work of these investigators, together with some few experiments of my own, had produced a feeling

of interest as to the efficiency of even the best arranged arresters as connected in ordinary practice. I was prepared to believe this to be a practical live case of a well-mounted and connected lightning arrester not protecting. It even seemed possible that the cable itself was, in reality, protecting the protector, when viewed in the light of Professor Lodge's "alternative path" experiment.

In order to come to some definite and practical conclusion as to the possibility of these troubles having been produced by lightning, a series of experiments was undertaken on lines somewhat analogous to those of Professor Lodge.

In Figure 1, the general and important conditions of a cable placed in the earth and provided with lightning arresters as usually connected, are outlined. In the figure, K represents the cable, L the line wire, and W the ground wire connecting the protector C to the ground plate G.

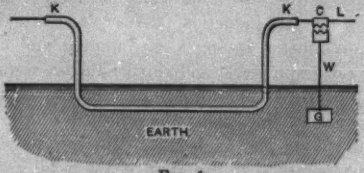


FIG. 1.

It is generally supposed that lightning—or an electrical discharge, as I prefer to call it—produced by the difference of potential between the cloud above the wire L, and the earth beneath, would, upon striking the wire, pass to the lightning arrester C, jump across the space separating the points to the wire W and thence to the earth, or the reverse, as the case may be. But while this is probably the case in many instances, it has not yet been satisfactorily demonstrated that there have not also been many cases where this plan of procedure was materially departed from, the discharge making a path for itself through the cable and the insulation, or perhaps dividing itself between both routes. In order to obtain more clearly defined ideas of the action of a discharge under these conditions, the apparatus shown in Fig. 2 was used, in which it will be seen that the fundamental features and the connections of Fig. 1 are reproduced for experimental work, the cloud being represented by the inner coating of the Leyden jar J, and the earth by the outer one, the charge being produced by a Holtz machine, of which M and N are the conductors. The

long length of the cable in Fig. 1 is replaced by a short one, K. As before, L represents the line wire, and in this case is connected

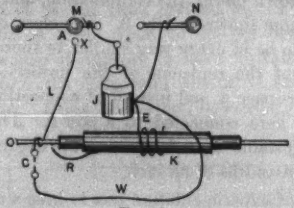


FIG. 2.

directly to the conductor, O, in the cable. The lightning arrester, is replaced by an equivalent, C, composed of two points separated an appropriate distance. The wire W is, as before, the ground wire connecting C in the former case to the earth, and in the latter to its equivalent, the outside of the jar; the lead covering of K is also connected by means of a short wire to the same outer surface. While in Fig. 1 the point on the line L which is struck by lightning is subject more or less to accidental conditions, it is in the case of Fig. 2 selected and adjusted by the ball X, as is also the value of the energy of action, by the areas of the surfaces on the jar J and the length of the striking distance, A.

With the apparatus arranged as just described and illustrated, it was quickly determined that a discharge could with great ease be caused to pass through the insulation of the cable, even when the space C was but a fraction of the distance through the insulation, and the length of W a few inches of stout copper wire. The current of discharge seemed to divide itself, a portion passing by W producing a spark at C, and another portion passing through the cable. A few more experiments also demonstrated that the wire W had apparently as much influence

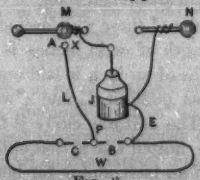


FIG. 3.

on the results as the space C, and consequently it became necessary to analyze the combination more completely to determine

the relative values of C and W. With this object in view, the cable K was thrown out of the combination, and the connections simplified to those shown in Fig. 3. On close examination they will be found to be in every way similar to those in Fig. 2, with the exception of the replacing of the insulation of the cable K with a clear air-space B, and instead of connecting L to the conductor O it is here connected to a post P. The spaces C and B were both adjustable to a fairly accurate degree, B being easily readable to thousandths of an inch.

The system of experimentation was to take a certain length of wire, W, adjust A and C to known values, and then adjust B so that one spark would occur there for about every ten at A, and after having noted the length of the spark B a few inches would be cut off from W and B readjusted and again noted; this being repeated until W was wholly consumed. The values of B for these various lengths of W and a known length of C having been ob-

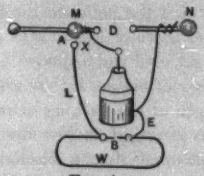


FIG. 4.

tained, a wire similar to the original W was substituted for the former, and C having been removed from the circuit, as shown in Fig. 4, a similar set of tests were made, and the differences between the results, with and without C, indicated the true values of C and W in the combination, while the results with W alone show the value of each additional inch.

In plate A the results of these measurements have been plotted into curves. The length of W in inches is plotted horizontally, while B in thousandths of an inch is laid out vertically. The capacity of the jar J was .0025 m. f., and A was retained uniformly at .286 of an inch. The full line curve is that of W without C in the circuit, while the dotted one is that obtained with it in. The length of C was .02 of an inch, and W for both tests was composed of a copper wire having a diameter of .052 of an inch.

A brief study of these curves teaches that, when W is but a few inches in length, C plays a very important part in determin-

ing the length of B, and also that its influence rapidly vanishes as w is lengthened, and actually produces an opposite effect after a certain length of w has been arrived at. Thus, with w 20 inches long, c has a value equal to 59 per cent. of the total circuit; at 110 inches it appears to have no value, while at 180 inches the value of the compound circuit is about 7 per cent. less than w alone. These points are, however, of even less im-

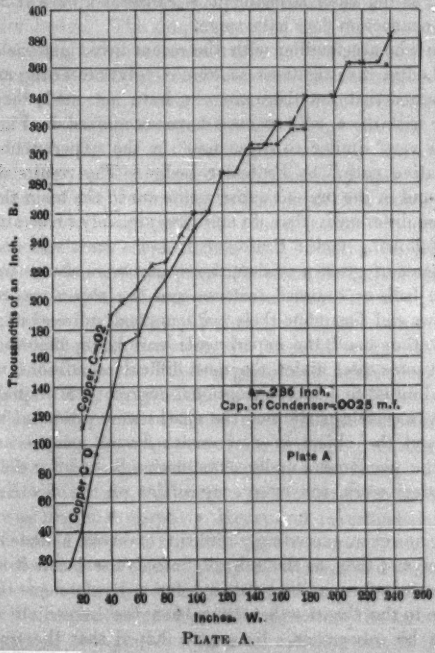


PLATE A.

portance than the lesson to be learned from the study of the curve of the results obtained from w alone. Here is a case where A relatively small amount of energy contained on the surfaces of the jar J will, when discharged into the circuit composed of the air space A and the wires L, W and E cause the passage of energy in the form of a spark at the point B of a length of over one-third of an inch, and this when the wire w is but 20 feet in

length, or that of an ordinary ground wire down a telegraph pole.

This does not look very encouraging for lightning arresters with long ground connections, and rather puts to ridicule the fine adjustment of the points of these devices. The subject will, however, stand more than a little study and investigation before forming judgment, and perhaps the arrester is all right after all, in so far as the close adjustment is concerned, even if the long ground connection does have to go.

Those who are familiar with the recent investigations of Professor Lodge, investigations so exceedingly interesting and now so celebrated that few there are who have not made themselves familiar with them, will at once have recognized in Fig. 4 connections very similar to those used in the experiment of the "alternative path," by Professor Lodge. The results obtained by him and in the present experiments are in the main the same, and those differences that do exist are probably to be attributed to the following facts: Professor Lodge's work was done with instruments of great precision, the sparks were always produced between balls or rounded surfaces, and the object was to determine laws and formulate their mathematical expressions; while, on the other hand, the experiments now being described were made in some cases under the most difficult conditions, with apparatus limited to a most exceptional degree both in quality and quantity, a certain portion of the sparks were produced between points, and the object in view was to dissect, analyze and subjugate the phenomena under examination to and for the benefit of practical work, the every day affairs of the electrical profession.

It is, however, exceedingly difficult to avoid a little speculation, now and then, as the subject unfolds itself, and it is hoped the experimenter may be pardoned for any references that may be made to the theories that have been the immediate cause of some of his researches. It is even hoped that the work performed may atone for the views about to be advanced, notwithstanding the fact that they are quite contrary to those deduced by so high an authority as Professor Lodge.

After a considerable portion of this work had been done and the conclusions more or less formulated, I was much pleased and greatly strengthened in the opinions then being formed to learn through a letter from Professor Hughes to Professor Lodge,

published in the London *Electrician*, issue of September 28, 1888, that Professor Hughes had, at least formerly, held the same views of the "alternative path" that I had formed, and which will now be presented in connection with what is thought to be experimental demonstration of their truthfulness.

Referring to Fig. 4, it will be seen, as has already been stated, the conditions and arrangements of w and B are such as to produce and actually constitute the "alternative path," as shown by Professor Lodge. The simple assertion that they are alternative paths does not, however, make them such, and, while they may under certain conditions and adjustments become so, it is possible to conceive of their being otherwise. It is not beyond the bounds of reason to assume that the energy that has been discharged from the jar J, traversing the wire w, produces in it a condition that may be termed *electrification;* and on the disappearance of the controlling influence of this energy, would not the wire w return with an almost infinite rapidity to its original non-electrified condition, and, in doing so, produce as the result of this enormous rapidity of motion, an electromotive force at its terminals of a greater value than that of the original energy? In other words, will there not be a restoration of the energy expended in distorting the lines of energy within the wire, and this restoration being performed in a shorter interval of time than that occupied in its production, will not a higher electromotive force be obtained even though the value of the return would be less than that of the original to an extent equal to the losses due to the internal friction of the mass?

This being the case, this current of discharge from the wire—which was first discovered by Henry and investigated by Faraday and called by him the *extra current*—would, owing to its greater electromotive force, jump the space B and thus close its circuit, even when the distance was such as to prohibit the passage of the original current as discharged from the jar J.

It seems as though this might be the case, and, if it is, then the discharge that was obtained through the insulation of the cable in Fig. 2 need not have been a part of the original current from the jar J, but the current discharged from the wire w. If it was, a shortening of w would tend towards overcoming the difficulty. To test the case a short wire B was attached to the lead and bent out over the insulation so as to present a point to the conductor O, the distance between the point and the con-

ductor being about the same as the thickness of the insulation; W was now removed, as was also C. It will be seen that E, the lead of the cable K, the short wire R and the space separating its end from the conductor O, form an equivalent to W and C. With this arrangement it was not possible to cause a discharge to pass through the insulation.

Now, in accordance with the views which have seemed to me most probably true, this change is due to the reducing of the length of the path W, and which is now composed of R, E and the lead of the cable, this deduction resulting in a subsequent lowering of the potential of the extra or self-induced current. But if the theory of Professor Lodge's "alternative path" is correct, then the change would be due to a removal from the path W of a certain amount of resistance, which has been termed by Mr. Oliver Heaviside, *impedance*.

We here have an apt illustration of inertia under the American and English definitions, as so forcibly stated by Professor Ayrton. As I have presented it, the spark B is the result of inertia, with the American definition—a resistance to stopping. While Professor Lodge's theory would account for it by inertia with the English interpretation of that word—a resistance to motion.

Accepting the belief that the spark B, when adjusted to the critical point, was caused by the self-induction of the wire W, a variety of experiments were made with the desire of obtaining experimental proof of the theory. Repeated occasions presented themselves for mathematical work, and if time and attention could have been given in that direction, the results would unquestionably have been of interest and value, but the work was perforce restricted to the more limited confines of experiment and reasoning. The first experiment of the series was to replace the wire W in Fig. 4 with one of smaller diameter, and re-determine the value of B in the manner already used for a copper wire having a diameter of .052 of an inch, and of which the results have been plotted in plate A.

Before presenting the experimental results, it is desirable to consider what might be expected from these changed conditions. There will in all cases be practically the same amount of energy discharged into the circuit, for the areas of the condensing surfaces on the jar J remain constant, the length of the striking distance A also remains uniform, and while it is true that the resistance of W, as measured by a Wheatstone bridge, would vary

inversely as the square of the diameter, the resistance of the complete circuit of discharge is, owing to the air space A, practically infinite, and hence the relatively trifling resistance of w, even when of the finest wire, may be wholly neglected.

The theory that the self-induced or extra current in a wire is a recoil-action, is based primarily upon the hypothesis that there exists, before the electrifying current is discharged into the circuit, a prior state—the *electro-tonic* state of Faraday. This *electro-tonic* condition consists in the existence of lines of energy—thermal they may be—with directions coinciding with the radii of the wire; these lines or axes of energy being surrounded by circular lines of magnetic force. An electrical current being discharged through such a wire would cause, under the influence of its well-known magnetic whirls or circles, whose planes are at right angles to the line of propagation and in this case to the axis of the wire, a deflection of the planes bounded by the closed lines of magnetic force surrounding the radial lines of energy which are within the wire. The work of producing this deflection would, I think, constitute what is known as the variable resistance which meets a sudden discharge, and which was made the subject of that grand paper on "Self-Induction" by Professor Hughes before the Society of Telegraph Engineers, at their meeting of January 28, 1886. This deflecting of the radial lines of energy, while constituting a resistance to the inducing current, is also, the source of, or rather, during the act of deflecting, produces, an electromotive force contrary to that of the inducing current. This counter electromotive force exists only during the interval of deflection, and quickly vanishes from the path of the inducing current. On the moment of the interruption of the inducing current, and consequent withdrawal of its attending magnetic whirls or circles, the deflected radial lines of energy swing back to their radial positions, producing by their motion an electromotive force of such a direction as to form a current in the same direction as that of the inducing current. This return swing or recoil, as of a spring suddenly released, is of such a velocity, and overlapping, probably, what might be likened to the tail end of the disappearing inducing current, produces a momentary electromotive force of a value higher than that of the original current. These being the actions of the forces and energies, it now becomes necessary to understand what differences, if any, the size of the wire through which the original current is

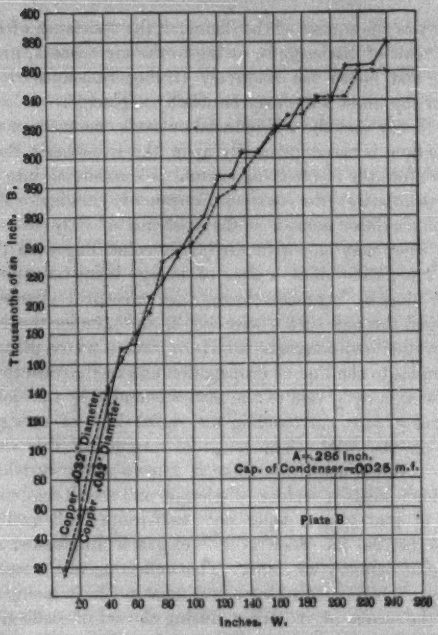


PLATE B.

discharged will have upon the value of the induced, or, as it is more generally termed, the self-induced electromotive force. All of the experiments and considerations will be made under the conditions of a constant capacity for the jar J and uniform length of A, and consequently one value for the energy discharged into the circuit. In all cases there will, of course, be a certain amount of the energy that has been discharged into the wire lost and converted into heat. This lost energy will be greater in value in the smaller wire than in the larger in a proportion inversely as their diameters. This greater loss in the small wire and resulting reduction in the available inducing energy would, of course, tend to produce a diminution of the self-induced current. This reduction is, however, exactly offset by the greater concentration of the inducing energy, its inducing value having been

increased in a proportion inversely as the diameters of the wires.

It is feared this argument has become tiresome, and an immediate relief will be found in plate B, where I have plotted two curves; one, the full line curve, is the same as that shown in plate A, and was produced as formerly stated. The dotted line curve was made with a wire of copper having a diameter of .032 of an inch. The two curves are so nearly alike as to permit of the belief that they are in reality the same, and that the slight differences existing are due to errors of measurement. Here is experimental proof that the electromotive force of the self-induced current, as measured by the spark B, remains constant as long as the energy discharged into the circuit is of uniform value, and is wholly independent of the diameter of the wire, and this seems to give countenance to the argument immediately preceding.

It is more than probable that those who have been following

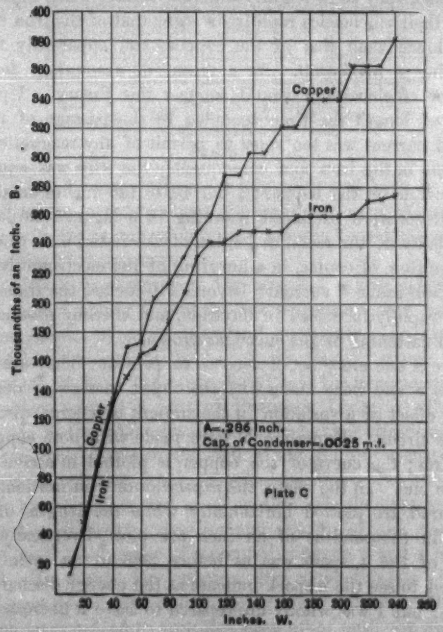


PLATE C.

the subject closely will, before this time, have had the question asked, from somewhere within: "How would these curves on Plate B have looked had one been made with an iron wire?" The answer is ready at hand on Plate C. It is not exactly an answer to the question as stated, for there are here shown two curves, one produced with an iron wire and the other with a copper wire, but both are of the same diameter, .052 of an inch. The curve of the copper wire is the one you are already familiar with.

This paper has even now attained such proportions, and there are yet so many points to present, that nothing more than a moment's glance at these curves can be offered. A study of them teaches that for lengths of 40 inches, copper and iron will, when acted upon inductively by discharges of one value, furnish self-induced currents of like electromotive forces. From 40 to 110 inches they show slightly different values, while beyond this length their efficiencies rapidly diverge; that of the iron running much lower than that of the copper, and apparently fast approaching a maximum. As a means of accounting for these different efficiencies, I would suggest this theory. Up to 40 inches of length the time occupied by the passage of the discharged current was too short to permit of any magnetic effects occurring in the iron, and consequently the wire was equivalent to its neighbor, the copper; from 40 to 110 inches small magnetic effects were produced, resulting in a lag or retardation of the velocity of the recoil deflection of the radial lines of energy, and resulting, of course, in a lowering of the electromotive force of the self-induced current; beyond 110 inches, the time of discharge rapidly increased in duration, and thereby produced the decided flattening of the curve, as shown.

There is one more set of curves to present: They are shown in Plate D, and were made with the object in view of determining the effect of a variation in the amount of energy discharged into the circuit. Experiments were made with both copper and iron wires; the curve of the copper is plotted in above that of the iron one. In the plate, the capacities of J in millionths of a microfarad are plotted horizontally, while the lengths of the B sparks in thousandths of an inch are plotted vertically. The length of the A spark was, as before, .286 of an inch. I was prepared to see the B spark increase as the energy discharged increased; but I was wholly unprepared to have it decrease as the

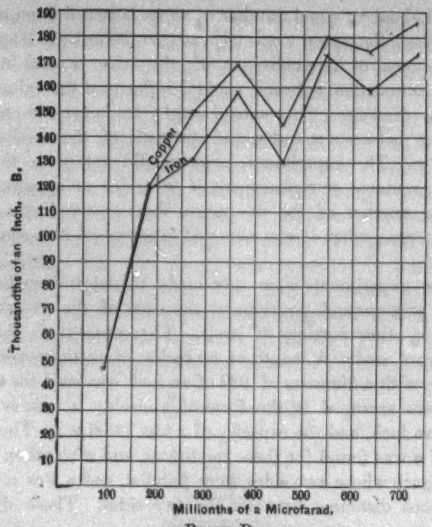


PLATE D.

result of added energy after a certain point had been arrived at, and still less prepared to obtain the wavy curves, as shown.

Time will not permit of entering into any consideration of the peculiarities of these curves.

There is a little experiment, the record of which I wish to interline at this point; it may answer a question, and perchance wet the powder of some intending questioner. Looking over my sketches and coming to Fig. 3, the thought struck me that an error might have been introduced into the experiment where the c spark formed a part of Professor Lodge's "alternative path," due to there being no connection having other than an infinite resistance between the ball x and the outer coating of J, while in all of the other experiments there was the circuit formed by w. It seemed possible that this change might have produced a different value for the energy of the discharged current, as the difference of potential between M and x might be altered. To decide the question, the connections as shown in Fig. 4 were used. The balls forming the terminals of the Holtz machine were caused to approach each other until the discharges

occurred about an equal number of times between them at D and at A. Next the space B was adjusted to the critical length. If the existence or non-existence of the wire W and resulting metallic connection between X and J influenced the value of the potential discharge, its removal ought to lower the potential difference between M and X, and cause all of the discharges to occur at D. The experiment, however, did not prove this to be true, the removal or replacement of W having no apparent effect upon the number of the discharges that occurred at A and D; but, of course, when it was removed every spark at A caused one at B.

A few experiments were now made to determine the effect various portions and positions of the parts of the wire W would have upon other portions of itself. The connections shown in Fig. 4 were used. A length of 50 inches of cotton-covered copper wire, with a diameter of .033 of an inch was used for the wire W. It was arranged in the form of a circle. A was, as before, .286 of an inch, and the capacity of J was .0025 M. F. The critical point of B was found for these conditions, and W was then formed into a figure whose two sides were parallel, and B was measured for various distances between the two sides. These distances were, however, only approximate.

Position of W.		B in inches.
Circle		.182
Sides separated	3½ inches	.170
	2½ inches	.137
	2 inches	.137
	1½ inches	.137
	1 inch	.137
	½ inch	.114
	¼ inch	.050

The next experiment, illustrated in Fig. 5, was to take the same wire, W, and coil it upon a paper tube having a diameter of ¼ of an inch, the wire forming a spiral of one layer about two inches in length. This spiral being placed in the position W the critical point of B was found to be .197 of an inch, or over 8 per cent. greater than when the wire was in a circle. While the spiral was in the last position an iron wire one-tenth of an inch in diameter was inserted into the paper tube, and now the spark B only measured .170 of an inch, over 13 per cent. less than without the iron wire, and more than 6 per cent. less than the plain circle. Another experiment was made like the last, with the ex-

ception that the iron wire was long and bent around into a closed circuit, but it did not alter the results. The lowering of the value of the self-induced current upon the introduction of the iron wire was to me at the time startling, but a consideration of the case showed that it should be so, for a portion of the energy of the inducing current was absorbed in producing work in this mass of iron, and of a necessity weakened the self-induced current to an extent measured by the work this extracted energy could and would have done in the wire W, had it not been withdrawn. I have, however, here a confession to make. I was all of the time considering that this energy was expended in producing magnetic effects in the iron wire until a friend who had called to see some of the experiments suggested that I insert a copper wire instead of the iron one. I did so. The effect was the same as that obtained with the iron wire. The explanation

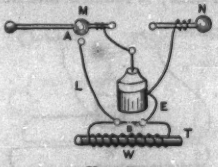


FIG. 5.

was simple: It was that not magnetic effects, but electric currents, closed upon themselves within the wire, had been produced. The reduction of the length of the spark B now became a simple question of causing the inducing current to do more or less work external to the wire W, and as another means of causing it to do so, the spiral was surrounded by another spiral of wire, with the ends so arranged that they could be connected or opened as wished. With this arrangement, the spark B could be made to come and go, as desired: for by adjusting it to the critical point with the terminals of the outer spiral open, it would entirely disappear when they were united, all of which agrees perfectly with the former experiments and conclusions.

Another short series of experiments was conducted with parallel circuits, as shown in Fig. 6. The lengths of the parallel portions were 35 inches, the distance separating them being .052 of an inch. The total lengths of L and W were 130 inches. A and J were of the same values as heretofore used.

The conditions of the tests and the resulting lengths of the spark s may be briefly stated thus:

Test No. 1.	L	was	copper,	diameter	.052 in.	S = .031 in.
	W	"	"	"	"	
Test No. 2.	L	"	"	"	"	S = .024 in.
	W	"	iron,	"	"	
Test No. 3.	L	"	"	"	"	S = .020 in.
	W	"	"	"	"	
Test No. 4.	L	"	"	"	"	S = .025 in.
	W	"	copper,	"	"	

If my memory serves me right these results are the same, qualitatively, as those obtained by Professor Hughes, and clearly show the apparent sluggish action of iron, both in imparting and receiving magnetic inductive effects. Its relations to these magnetic effects resemble the actions of a polished metal surface when associated with heat; it radiates and absorbs poorly.

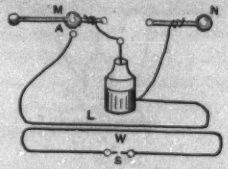


FIG. 6.

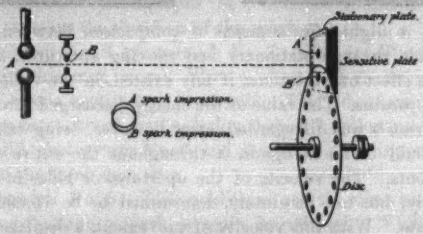
It now becomes my pleasant duty to present the concluding and, at the same time, most interesting and convincing proofs that W is not an "alternative path," but that the spark B is produced by, and consists of, the discharge of the self-induced current. I have called this series of experiments

THE PHOTOGRAPHIC STUDY OF SELF-INDUCTION.

Unfortunately, at a late day, a day so late as to prevent my repairing the loss, I met with at least two accidents, and each of them cost me one or more negatives which were a part of the record of these experiments. I shall, however, endeavor to make those remaining demonstrate the desired points.

In making these experiments the end in view was to obtain photographic records of the duration of the spark A, with and without the spark B; with and without the wire W; with various lengths of the wire W, and, if possible, to detect a difference of time between A and B. A disc of tin plate, 20 inches in diameter, was mounted on an arbor and belted to a system of pulleys of

such sizes that by working the train by hand a velocity of about 75,000 inches per minute could be given to a circle of holes that had been punched in the disc at a distance of one inch from its edge. These holes were about one-eighth of an inch in diameter and one inch apart from centre to centre. The disc was so placed in relation to the positions of the A and B sparks that a line stretched from A to one of the holes in the upper edge of the disc would pass through B. Immediately back of this same hole a sensitive plate was fixed so that the hole was in front of the centre line, while the plate was removed sufficiently far to avoid being touched by the disc. One-half of the sensitive plate projected above the edge of the disc; this upper portion was covered with a tin plate in which a hole was punched of a size similar to those in the disc. The lower portion of the sensitive



APPARATUS USED IN EXPERIMENTS.

plate—that is the part back of the disc—was also covered with a tin plate in which a window had been cut a little over one inch square, and in such a position that the holes in the disc passed in front of it. It will be understood from this description, that while the disc revolved there could be two holes in front of the window, while there could not be less than one. With these adjustments sparks at A and B, separately or united, would produce an impression upon the sensitive plate, through the upper hole, and through one or two holes, as the case might be, in the revolving disc.

As a result of the accidents above referred to, it will be necessary to introduce the series of experiments with photograph No. 5, where we have the spot produced through the hole in the upper stationary screen marked A, and a spot lower down on the plate marked B, and which was produced through one of the holes in the revolving disc. The object in having the spot A

was that it might offer a means of comparison between impressions made through stationary and moving apertures, and also show the effect of vibrations, if any existed, in the various parts of the apparatus. The value of the energy discharged through the circuit from J was disregarded, care, however, being taken that there should be no change in it throughout the entire series of experiments. The velocity of the apertures or holes in the disc was fairly, but not absolutely, determined to be 75,000 inches per minute. With this velocity of movement, a displacement of the apertures of one thousandth of an inch would represent an interval of time equal to $\frac{1}{75000000}$ part of a minute.

The series of experiments will be divided into four sets as follows:

Experiment 1, photograph No. 5.—The wire W was of copper, 76 feet long and .052 of an inch in diameter, the spark A was $\frac{4}{10}$ of an inch and was retained at this for the entire series.

The B spark was adjusted to a point near the critical state, but, owing to the necessity of its production with the first A spark, it could not be set to the same delicacy of action as heretofore. The photograph shows a slightly blurred or indistinct outline on the right-hand side of the spot B, which is more clearly brought out by comparison with the spot A. From this it is concluded that a measurable length of time was occupied by the spark, as the aperture in the disc through which this B spot was produced must have moved an appreciable distance in order to have produced this elongation or "ghost," as the photographic profession would

term it. Owing, however, to the absence of any displacement of the distinct outlines of the spot, it cannot be assumed that there was any difference in the time of A and B, or, at least, not of sufficient value to permit of measurement with this apparatus.

Photograph No. 6.—In producing this photograph all of the conditions remained the same as those used in photograph No. 5, with the one exception of the spark B, which was removed from the circuit by separating the points beyond the striking distance. Here, by chance, two holes, or rather one and a half, were in front of the window at the moment of sparking, and produced the spots one and two. These spots exhibit displacements that, roughly measured on the negative, are one-tenth of an inch in length, and show a duration of the spark of not less than $\frac{1}{72000}$ of a minute.

From these two photographs the conclusion is drawn that the spark B shortens the duration of the spark at A.

Experiment No. 2, photograph No. 7.—The conditions of the circuit in this case were: Wire w shortened to 20 inches in length; spark B still out of circuit. This photograph shows about the same length, but a darker "ghost" following the spot B than did photograph No. 6, and indicates a corresponding increase in the *work done at* A.

Photograph No. 8.—The conditions are the same as the last, excepting that the B spark is again introduced. While there is still a measurable duration to the spark, the shortening of the interval due to the spark B is very decided.

Experiment No. 8, photograph No. 9.—In this experiment the entire apparatus associated with the spark B was cut out and the outer surface of the jar J was connected by a short copper wire to the ball X (refer to Fig. 4). An examination of the photograph indicates that A existed for an interval of time approximately equal to that of the last experiment (photograph No. 8) and very much less than that shown in photographs Nos. 6 and 7.

Photograph No. 10.—In this case the wire W was removed and the spark B adjusted to about $\frac{1}{10}$ of an inch. There seems to be, possibly a very slight increase in the time over that of photograph No. 9.

Experiment No. 4, photograph No. 13.—To this photograph special attention is invited. The conditions under which the negative was produced were wholly different from the foregoing. The tin plate covering the upper half of the sensitive plate, and which carried the aperture through which the spots A were made, was removed, the plate was lowered, so that a point represented by the spot marked 1 came on a line with the apertures in the disc. The wire W was composed of 50 inches of cotton-covered wire like that used in some of the earlier experiments, and was formed into a circle. The B spark was removed and was not used during this set of experiments. The A spark was, as before, $\frac{1}{8}$ of an inch, and the spot 1 in the photograph is its impression through an aperture of the disc, and, as readily seen, shows a very considerable duration of time.

The sensitive plate was now elevated a little, the wire W was coiled into a spiral, as described in a former experiment, and the spark A was once more caused to pass. Spots 2 and 2′ are the impressions produced by it, and, as will be seen by close inspection, exhibit longer "ghosts," and consequently a greater interval of time, than was shown in spot 1. Once more the sensitive plate was moved upwards, and an iron core having been inserted into the paper tube upon which W was coiled, the spot 3 was produced by another spark, the "ghost" produced being smaller than either of the former ones. These three experiments show conclusively that the presence of the wire W in the form of a loop or circle in the path of the discharged energy, tended to prolong the action upon the sensitive plate; next, that the coiling of the wire into a spiral increased the duration of that action, and lastly, that the introduction of the metallic core into the coil, and consequent stealing away of a portion of the discharged energy, shortened the time of action to an interval less than that of the loop.

The field opened up by these photographs for theorizing and speculating is a very broad and rich one, but the time is now too short to more than indicate the manner in which these results add strength to the conclusions deduced from the former experiments. Applying to the photographic experiments the theory already advanced, that the B spark was a discharge from the wire W, it is found, as would be expected, that when B is absent the self-induced current discharges into the jar J, producing in its passage a spark at A; this spark following immediately upon the

heels of the original A spark. The jar being in this manner recharged, but, of course, in the opposite direction and of a slightly lower potential, will again discharge into the circuit—the discharge at a reduced potential being facilitated by the thermal and, possibly, magnetic conditions of the air space A, and which were produced by the first spark.

This second discharge will leave the jar J once more charged, and will be followed by other discharges of a continually decreasing value, until the energy has been dissipated as heat or into other forms of energy through and by the various portions of the circuit. This subject of the oscillating discharge has lately received, at the hands of Dr. Hertz, a masterly and most highly interesting treatment.

Referring once more to photograph No. 6, which was produced by an A spark with w composed of a copper wire 76 feet in length, the impression produced by the original, or first spark, is clearly defined, while in its wake is traced the track left by the sparks produced by the rapidly weakening, oscillating, self-induced currents—a track fading away like the tail of a comet, to an invisible end.

The next step in the inquiry is to determine the effect of introducing the spark B, and for this purpose reference will be made to photograph No. 5. Here we find, as would be expected, that the comet-like tail has almost disappeared, the natural result of having offered the self-induced currents from the wire w, a means of short-circuiting themselves across the points at B. The slight trace of a tail—a blurred outline—is to be attributed to the self-induced currents of the portions of the discharge circuit lying between the points of B and the surface of the jar J.

Shortening the wire w, with the spark B removed, should have the effect of increasing the amount of energy dissipated in the air space A, and further than this there should be little or no alteration. This increased amount of dissipation at A may be popularly explained thus: The total circuit into which the energy contained in J is discharged, and which consists of the space A, the wire w and the connecting wires, constitute the bodies of spaces from which the energy is radiated or dissipated, and, therefore, any contraction due to removing a portion of them will, of necessity throw more work upon the parts remaining. That this is correct is illustrated by photographs No. 6 and No. 7, where the conditions were the same other than a shortening of w from

76 inches in No. 6 to 20 inches in No. 7. A comparison shows that while the length of the "ghosts" are practically the same, that of No. 7 is heavier—more material. In the same manner in which the work was increased in the space A, so will it be increased in the wire connecting the points at B to the surfaces of the jar J, and for this reason the introduction of the spark B should not be expected to cut off the tail from the spot B to the same value obtained with a long wire at W. This is the case, as is shown in photograph No. 8, where W is 20 inches and B has been returned to the circuit. If the "ghost" shown in photograph No. 8 was due to the self-induced currents in the connecting wire, as has been assumed, then the same results should be obtained by cutting off the entire portion of the circuit represented by W and the apparatus associated with the B points, and short-circuiting these connecting wires. That this is the case is shown in photograph No. 9.

Cutting out W and replacing B did not seem to alter the results very much, if any, from those last mentioned, and this is just what would be expected.

In photograph No. 13 it is demonstrated that the coiling of a wire into a spiral tends to cause a retention of the energy within itself, and is due probably to the gathering up of a portion of the energy radiated or dissipated by one convolution by the adjoining convolution, and thereby restoring it to its own mass. This retarding of the process of dissipation will result in a prolongation of the time of A and consequent action on the sensitive plate, as is shown by the increased length of the "ghost," following spots 2 and 2′ over that following the spot 1. As would be expected, the length of the "ghost" is immediately contracted upon increasing the rate of dissipation of the energy as results from the introduction of a metallic core in the coil of W, as is illu-trated in spot 3.

The final conclusions to be drawn from these experiments are:

1st. The "ground plate" of a lightning arrester should be connected to earth by as short a connection as may be possible.

2d. When an air line connects to a cable, a lightning arrester should be provided and attached to the armor of the cable by a connection not more than a few inches in length.

3d. When a short "ground wire" can not be used, one formed of a number of strands may be used, preferably not twisted into a cable.

4th. Any method by which a portion of the discharged current which passes over the "ground wire" may be withdrawn, will tend to increase the efficiency of the arrester.

5th. Under conditions similar to the tests, copper or iron may be used with equal efficiency, when the lengths are under forty inches, and over this length iron is more efficient than copper.

6th. The resistance of the "ground wire" is of little consequence, but it should have a size sufficient to carry off the charge without overheating.

7th. The insulation in a cable may be punctured by lightning, either by the direct discharge, due to the points of the arrester being too far apart, or by the current of self-induction from the "ground wire," when that wire is of any considerable length.

8th. Dissipation of the energy of discharge reduces the self-induced current.

DISCUSSION.

THE CHAIRMAN (Mr. Francis W. Jones):—Gentlemen, you have before you a very interesting subject. It is unnecessary for me to say how well and ably it has been presented. I will not take any of your time, but lay the subject open to your consideration and for any remarks the members may wish to make. It is understood, of course, that all present are at liberty to take part in the discussion. We should be pleased to hear from Dr. Moses.

DR. OTTO A. MOSES:—Gentlemen, as I have had a very good opportunity of knowing the careful methods in which Mr. Acheson directs his thoughts experimentally, I feel to a certain extent diffident upon being called upon so suddenly to express any doubts as to the correctness of his deductions. He has conferred with me occasionally on the subject in the course of his experiments. There are one or two things that might tend to start this discussion which I will suggest, so that some who are present here, who I know have taken great interest in this matter may continue it.

In the space A did you consider the difference in the size of the condensing surfaces of N and A?

MR. ACHESON:—Yes; I attempted to make a measurement of the capacity of the apparatus with the jar removed; but although I had a constant on the galvanometer—a very high constant—I failed to get any capacity, not because there was none there, for

we knew that there was, but it was beyond the reach of my instruments. The size of the balls was considered, of course, and the ball x was made of a size that would allow me to obtain a sufficiently high electromotive force without spinning off, as it would do where it was very small.

Dr. Moses:—Would you be so kind as to enlighten us as to the nature of the apparatus with which you made these speed determinations? You condensed the explanation a little. Would you kindly tell us in what way the experiment was carried out?

Mr. Acheson:—You mean the spark length? I thought I had described that probably as fully as I am able to. I do not know whether I could exhibit it more fully without the actual apparatus.

Dr. Moses:—Let me ask a few questions. By what means did you cause your rotations?

Mr. Acheson:—By hand. I used, in fact, Mrs. Acheson's sewing machine as a motive power.

Dr. Moses:—In plate No 13, when you shifted the machine—

Mr. Acheson:—Well, I shifted the position of the sensitive plate, my revolving disc remaining in its former position. As the disc was rotating, immediately back of it was my sensitive plate, and in order to produce this first spot marked 1, I had the plate at a certain height and raised it higher for the succeeding ones.

Dr. Moses:—Has it ever struck you that perhaps these experiments might have great bearing on the subject of sparking in the brushes of dynamo machines?

Mr. Acheson:—Well, I don't know but that they have.

Dr. Moses:—By inserting shunts between the brushes, looping the brushes together instead of making them direct in contact so as to be able to time the sparks. It would be a very interesting thing for you to experiment upon. Your power there of avoiding the spark might enable you to avoid the spark on the brush, which comes evidently from the same cause. In the paper in which Prof. Hughes describes these experiments to which you refer, he indicated that there might be some difference in the metals that were used for brushes. There had been experiments carried on in this country before then in that direction; but it seems to me it would be quite a prolific field of investigation to find out whether by alternating brushes, perhaps of iron and copper, you might not be able to eliminate the spark from the dynamo machine.

Mr. E. T. Birdsall:—Dr. Moses speaks of sparking from the brushes of dynamos. It has been some time since we have seen much of that.

Dr. Moses:—There is a gentleman here who represents an interest in the city of New York where very considerable dynamo sparking can be seen.

Mr. Birdsall:—I represent electrical interests, and I have seen dynamos, I guess half a dozen different makes, that did not spark; also motors, which are practically the same thing.

Dr. Moses:—You have heard the anecdote of the man whose lawyer told him he could not be put in prison on account of a legal technicality and who wrote to his lawyer saying that notwithstanding that, he was actually in prison. I think if he looked, even without a microscope, at the brushes of the dynamo machines, he would find sparks.

The Chairman:—We have with us a gentleman from Boston, Mr. Hamilton, from whom we should be pleased to hear.

Mr. Leonidas Le C. Hamilton:—I have been very much pleased to hear the paper which has been read to you this evening. I do not know that I could add anything whatever to what has been said already. One question arises in my mind—whether or not the ghost, so called, in the photograph might not have been caused by a difference in the revolution of that disc. It seems to me that the experiments, with no possibility, as I can understand it, of accuracy, as to velocity of revolution, might possibly result in this ghost filling the plate.

Mr. Acheson:—I neglected to state the manner in which the disc was timed. I timed it always. Of course, I could not get an exact speed. These are not printed exactly like the negatives. While some of them exhibited a little or no ghost at all, others exhibited a length of as much as a tenth of an inch. Now, we know there is no difference in speed like that.

Mr. Hamilton:—How did you control the speed?

Mr. Acheson:—Well, it was pretty nearly the limit of my power. I had everything ready for the discharge of the jar and held that in waiting until I had the speed up, with my watch before me. I worked with a treadle so that I had perfect control of it and worked it up until I had a revolution of the band wheel of a certain value, of which my note book gives the record in each and every experiment.

Mr. Hamilton:—The photographs were each separate, were

they not? For instance, in No. 5, A and B were the photographs taken at different times?

MR. ACHESON:—The A and B spots were made by one spark at the same instant of time. The spot A was produced through a stationary aperture, while the spot B was produced through the moving aperture in the disc. It was a question that I wanted to decide for my own interest to determine whether any vibration that might be introduced in the work would cause any movement of the outlines, and I always had the A spot for that object so that I might have a means of comparison with the spot produced by the moving aperture. I am sorry that these are not an exact print of the negatives, but I presume we never will have them. I do not suppose that they could be reproduced exactly. Even if the plate is good, the printing will disfigure that; it will not always be the same, and the differences between them are very slight at most.

MR. LEMUEL W. SERRELL:—I would like to ask Mr. Acheson if he developed the negatives all at the same time; that is, with the same developer and gave them all an equal length of time in developing. This ghost, or tail of a comet, is a vanishing thing and if the development was applied at different lengths of time it would make a different depth to the negative and make a different length to the ghost or to the tail. I would like to know whether they were all developed at the same time.

MR. ACHESON:—I am not a photographer by profession, and in fact I never did any photographing until I commenced this. I never had an instructor. I bought my supplies in a store and used a small hand-book as a guide. I think I spent probably fifteen or twenty dollars in buying developers and plates until I accumulated quite a stock. Although I spoiled some plates, each and every one of them was developed separately, and it was all done after night. None of this work was done by daylight. Much of it was done in the neighborhood of midnight, or later; as a rule, after considerable experience, I developed them until they ceased to show any increase.

MR. SERRELL:—I would like to say that Mr. Acheson got off pretty cheap at fifteen or twenty dollars, for a beginner.

MR. ACHESON:—Well you must remember I had only a spot here to deal with.

MR. BIRDSALL:—In taking pictures on glass plates, such as Mr. Acheson took, although these probably do not exhibit that defect

to any extent, a glass sensitive plate, pointed at any bright object and a picture taken of the bright object, will always exhibit this halo, and taking a picture through a hole like that, if the source of light were not perpendicular to the surface of the plate, it would exhibit that halo on one side. If the spark was not far enough away from the surface of the plate so that both the holes A and B were practically in a perpendicular line, that property which is called halation of plates, and which is avoided in the paper negatives, would introduce probably an appreciable error, as in taking pictures with, say, a 10-inch focus lens, we get a picture of the sun on a plate about one-quarter of an inch in diameter and there will often be a half inch of this halo around this image on the plate. If we take a picture of the sun through a large hole, it would, in all probability, show more halation on one side than on the other similar to these pictures, although that error may not be introduced in these.

MR. JOSEPH WETZLER:—There is one point that occurs to me in connection with plate 5. Mr. Acheson remarked that when he applied an iron rod in that coil the spark was very much diminished. He attributed this at first to the magnetization, but when he inserted the copper he found the same diminution of spark. He then came to the conclusion that the retardation was due to the energy abstracted from the circuit by electric currents set up within the bar. It seems to me that that result of the equal action of the iron and the copper might have been predicated when we consider that it takes some time for iron to magnetize; in other words, the iron would not, as a rule, magnetize with a current of a static discharge on account of its great rapidity; that static discharge is practically instantaneous, while it takes some time for iron to magnetize, something in the neighborhood of one one-hundredth of a second, if I remember rightly. We might, therefore, conclude that the spark did not magnetize the iron, but was probably due to electric currents set up in the iron.

MR. ACHESON:—I quite agree with Mr. Wetzler in regard to those opinions and they are such as I finally formed. It was wholly due to the wonderful rapidity of the discharge and the want of time for it.

A gentleman brought up a question in regard to the possibility of a production of those ghosts due to the line of the ray of light not being perpendicular to the glass surface. At first I had some

difficulty, and thought I had achieved success before I did, owing to the fact that my two sparks and the hole through which they were photographed were not in a direct line, and it caused a distinct overlapping of the outlines of the spark, as produced in the spot, very distinct and sharp, At first I thought it was a difference of time between the two sparks; that the B spark was a little back of A, which was what I wanted, and I was much pleased; but it was not the case; it was simply due to the fact that they were not in line. The relative positions of the A and B sparks are shown in the diagram of the apparatus (p. 91). The A and B sparks are in a direct line. Now, I divided the difference between the apertures so that the line through A and B would strike about midway between them. If I caused it to strike one, it would produce a distinct difference in the spots as can be seen on some of my negatives, although I tried to get them exactly balanced; under no conditions could I get this ghost. If that was due to the causes which were suggested, it should appear in the spot A, but it is not there.

PROF. WM. E. GEYER:—I was unfortunate in not hearing the whole of this paper, and I am not sure that I fully grasped it. I hope I may be pardoned in asking a question for my own information—to what cause, Mr. Acheson, did you ascribe the ghost? I am not sure that I quite caught that.

MR. ACHESON:—It is due to the oscillation of the current and the continuous charging and discharging of the condenser surfaces, which has to go through the space A, as illustrated in these figures, producing a spark continually diminishing in flow and continuing for a certain length of time until that energy is expended in some manner. On the negatives you can see that the spark weakens and fades away until it actually disappears. You cannot give it a definite length. You cannot set an end to it. You do not know where it ends and nothing commences, due to that diminishing of the amount of energy which is oscillating backward and forward in the circuit.

PROF. GEYER:—I know of an analogous experiment which I think proves that the position you take is perfectly correct, and that it is not necessary to find photographic causes for the ghost. The experiment, I recollect, was one made by Prof. Meyer of the Stevens Institute, where a Leyden jar was discharged through the prongs of a tuning fork, at the end of which there was a little pointer which again moved over a rotating cylinder black-

ened with lamp-black. When the Leyden jar was discharged there was made on that blackened paper one great blotch where the lamp-black was dissipated. At this same distance further on, this cylinder rotating with uniform velocity, there was another blotch, and then there were three and four, and then there would be a moderate interval; then there would come a great group of them, and then another group, so that taking the trouble to count them there were actually several hundred of those discharges recorded, with no possibility of any photographic disturbance on this cylinder, showing that there were at least 700 of these oscillatory discharges.

MR. CHARLES CUTTRISS:—I would like to ask one point from Mr. Acheson in regard to these diagrams as taken from the negative. You will observe in both of them that there is a full light circle and then a tail. Now, I would ask if that was so on the photograph or was it not a gradual broadening out without any abrupt line between the bright part and the other? It would appear to me as though there was at the first discharge a greater volume of light or a more intense passage of current just at that distance to photograph the true hole and show it across the table.

MR. ACHESON:—If I understand the gentleman correctly he wants to know whether the outline of the aperture in the disc is clearly defined. It is, if that is the question. On the negatives it is clearly defined. You can trace perfectly all those outlines. I think that you are correct in assuming that the first discharge is vastly greater than any succeeding one. That outline is shown on the negative.

THE CHAIRMAN:—It is suggested that Dr. Garratt would favor us with some remark on the subject.

DR. ALLAN V. GARRATT:—Mr. President and Gentlemen: I have listened with a great deal of interest to this paper and discussion, and I cannot help wishing that before we go away we may get a few more facts in regard to these photographs, if possible, to clear up any doubt as to their good character, possibly. I should like to ask Mr. Acheson if the plates were all of one make.

MR. ACHESON:—Yes, sir; they were all of one make.

DR. GARRATT:—And all of the same number?

MR. ACHESON:—Yes, all number 26.

DR. GARRATT:—I understand they were developed practically until they would not develop any more?

MR. ACHESON:—Yes, sir; as near as I could tell.

DR. GARRATT:—I have occasionally monkeyed with plates myself. I see no reason under those circumstances to suppose that the trail of light, which I think a photographer would not call a ghost, is not correct. I do not see any elements of error there. In regard to the speed of rotation, some of these are only half the length of the others. I do not know with what degree of regularity Mr. Acheson can revolve a sewing machine wheel, but I should suppose that he would be able to eliminate an error of fifty per cent.; that is, if he worked it as hard as he could, and if he was in equally vigorous condition during all of his experiments, I see no reason to suppose that a very considerable degree of error was introduced there.

I cannot help feeling, gentlemen, that this paper will lead to a good deal of discussion and investigation on this extremely interesting subject. I believe that it is one that we shall hear a good deal more of.

MR. E. T. BIRDSALL:—My suggestion in regard to the spot of light showing the entire circle of the hole and then fading away, is that the first spark of the continued series of sparks which formed the discharge might be the one which has the most energy in it in the form of light due to conditions of the air, such as burning the dust in between the poles, and the succeeding ones might have as much energy in them, although not so much of that energy in the form of light, but more in the form of heat or other forms of energy.

MR. SERRELL:—I would like to ask Mr. Acheson which way his wheel turned around; whether the tail was due to the wheel gradually cutting off the light on the plate and the spark being produced by duration, or whether the first bright spot was due to the first flash and the wheel went in the direction of the tail.

MR. ACHESON:—When you look at this sheet, assuming that the disc is between you and it, the disc was revolving in a direction contrary to those poles, and by the photographic process they became reversed here; the aperture moved in the same direction in which the tail faded.

MR. HOLBROOK CUSHMAN:—I would like to ask Mr. Acheson whether the photographs through his moving disc were always photographed through a permanent diaphragm.

MR. ACHESON:—It is the A spark when it was formed alone

that produced both spots A and B. If the A and B sparks were together they united and formed these two spots; but the A spark always assisted at least in forming both the A and B spots. When the B spark was produced it likewise formed a portion of each.

THE CHAIRMAN:—This certainly is a subject in which gentlemen in almost all the various branches of the electrical profession have some interest, particularly in the department with which I have identified myself, where we find these antagonistic currents in our relays, in our cables and in our wires. It causes us a good deal of trouble to find just how they act and also to overcome their bad effects, particularly in relays where the signaling current, passing through the various convolutions, arouses this antagonistic current and causes relays to act very slowly. It changes the character of the signals entirely. I think probably, as suggested, it occurs in dynamo machines. This current, in fact, has a great deal to do with almost every electrical apparatus. I do not see how you can avoid it. I do not know that I am ready to express any opinion as to the basis upon which the able lecturer has given his opinion to-night. I think, however, that he is pretty nearly correct as far as I have reasoned out the thing up to this point.

If you have no further remarks to make upon the subject, the Secretary has some matter which he wishes to bring to your attention.

THE SECRETARY:—Gentlemen, it gives me pleasure to announce that the next paper to be read before the Institute in the regular series will be by Lieutenant F. Jarvis Patten, a Member of the Institute, on February 12th, entitled, "On a New System of Multiplex Telegraphy," with illustrations.

MR. CUTTRISS:—If the discussion on this paper is ended I would take pleasure in moving a vote of thanks to the author for one of the most able papers, and showing the deepest insight, I think, into recent research, that we have had before us for some time.

The motion was carried.

THE CHAIRMAN:—I have therefore very much pleasure in presenting Mr. Acheson with the unanimous thanks of the Institute, for the able and interesting paper he has presented with so much pains and so much care and accuracy of experiment.

MR. ACHESON:—I thank the Institute for this kindness in appreciating a little work.

Adjourned.

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NAME.	ABBREVIATION.	PUBLISHED, &c.
Engineer.	*Engr.*	London, 163 Strand, weekly. £1.9.
Engineering.	*Eng.*	London, 35–36 Bedford St. W, C.; weekly. £1.16.
Industries	*Indust.*	Manchester, 70 Market St., weekly. £1.12.
The Electrician.	*Lond. Elecn.*	London, 1 Salisbury Court, E. C.; weekly. £1.2.
The Telegraphic Journal and Electrical Review.	*Lond. Elec. Rev.*	London, 22 Paternoster Row, E. C.; weekly. £1.1.8.
The Electrical Engineer	*Lond. Elec. Engr.*	London, 139–140 Salisbury Court; weekly, 17s. 4d.
Nature.	*Nat.*	London, 29 Bedford St., W. C.; weekly, £1.8.
The Electrical Engineer	*N. Y. Elec. Engr.*	New York, 11 Wall Street; monthly, $3
The Electrical World.	*Elec. World.*	New York, 168–177 Potter Building; weekly. $3
The Electrical Review.	*N. Y. Elec. Rev.*	New York, 13 Park Row; weekly. $3.
The Electric Age.	*Elec. Age.*	New York, 5 Dey Street; semi-monthly, $1.50.
Modern Light and Heat.	*Md Lt. and Ht.*	Boston, 146 Franklin Street; weekly, $3.
Western Electrician.	*West. Elecn.*	Chicago, 6 Lakeside Building; weekly, $3.
Electric Power.	*Elec. P'wr.*	New York, 150 Broadway; monthly, $3.
The Electro-Mechanic.	*Elec. Mech.*	Kansas City, Mo.; semi-monthly, $2.
Practical Electricity.	*Prac. Elec.*	Boston, 620 Atlantic Ave.; monthly, $1.
Journal of the Franklin Institute.	*Jour. Fkln. Inst.*	Philadelphia, Franklin Institute; monthly, $5.
Science.	*Science.*	New York, 47 Lafayette Place, weekly, $3.50.
Scientific American.	*Sci. Am.*	New York, 361 Broadway; weekly, $3.
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Railroad and Engineering Journal.	*Rd. and Eng. Jour.*	New York, 45 Broadway; monthly, $3.

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